AGU Advances

Peer Review History of

Significance of diapycnal mixing within the Atlantic Meridional Overturning Circulation

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Author Response to Reviewers

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Reviewer #1

Review of "Significance of diapycnal mixing within the Atlantic Meridional Overturning Circulation", by Laura Cimoli et al, submitted to Advances.

The authors provide a multi-approach and largely observation-based analysis of the diapycnal mixing and associated water mass transformation and tracer propagation in the Atlantic Ocean. They found an overall buoyancy gain for North Atlantic Deep Water south of 24{degree sign}N and estimate a resulting diapycnal upwelling of up to 4 Sv. They finally attempt to estimate a typical vertical diffusive length scale, and investigate the vertical transfer of tracers and buoyancy and their associated timescales in a zonally-averaged numerical model of the meridional overturning circulation.

This is a concise article, well written and organized, and with clear take-home messages. It is largely based on the combination of already-approved methodologies for deriving dissipation rates or diffusivity estimates from observations. Those are here applied to the particular Atlantic Ocean in order to infer the importance of diapycnal mixing within the deep limbs of the overturning circulation. I do not think that the following comments will require substantial revisions, so I am here asking for "minor revision". However, several points deserve more care I believe.

1. I have appreciated the qualitative discussion about uncertainties, but I missed a quantitative feel about them. There is, for instance, a striking discrepancy between the inverse model estimate and the other three (Argo-based, strain-based, or tidally-driven). Part of the discussion (l. 471-498) seems to imply that the former is some kind of truth to which the later ones should be compared, notably because the data or model underlying the local estimates cannot capture all processes near steep and rough boundaries (whereas the bulk estimate implicitly includes them). The uncertainties of the bulk inverse estimate can also be pretty large, however (synoptic transport estimates, biased air-sea fluxes, ...). In fact, I am wondering if an uncertainty could be added to this estimate (one that would be propagated from the uncertainties usually provided as outputs by the inverse model). One might expect an uncertainty on the transformation rates nearly as large as the signal... I would also suggest to not only provide the ~4 Sv inverse estimate in the abstract and key points, but rather the range of maximum rates obtained from the 4 approaches (0.8 Sv - 4 Sv).

2. The time-spans covered by the four estimates are not mentioned and discussed in the text. For instance, I imagine that the inverse estimate is largely WOCE-based (1990's) while the other two are probably more representative of the latest two decades (e.g. Argo, or microstructure). Would the difference in the diapycnal mixing estimates be partly related to decadal AMOC variability (e.g. stronger state in the 1990's than in the 2000's in the North Atlantic)?

3. On the topic of the diffusive length scale, I wondered how sensitive the result was to the estimate of the transit time? I understand that the latter calculation is rather a "back-of-the-envelope" scaling (e.g. use of single-latitude decadal-mean velocity profile), but maybe more should be said on the underlying calculation because the authors build part of their contextualization/interpretation on this vertical length scale. I was particularly surprised by the very long residence times used (up to 350 years), given the approximate mean speed of the DWBC measured by currentmeters (5-10 cm.s-1) or mean basin-wide velocities derived from tracer measurements (2-2.5 cm.s-1), and which would give transit times of a few tens of years at most. Agreement with Arctic and Antarctic ice cores are mentioned without an explanation of their pertinence for a 48{degree sign}N-32{degree sign}S transit time. Also, would it be possible/useful to refine the length scale estimate by separating interior and boundary regions, with weak and strong mixing rates and meridional velocities, respectively? The authors suggest that this is possible for the tidally-driven estimate (l. 233-236). Finally, are those vertical length scales in line with recent tracer release experiments (e.g. BLT) or older ones (e.g. Brazil Basin)?

Some other comments:

Figure 1: I am guessing that neutral density is used here but it is not specified.

l. 142: some supplementary materials are mentioned here, but I cannot find any with the submission (I am guessing those were included in an earlier submission - hopefully I did not miss key information here).

l. 155-156: why is this improvement limited to below 2000m depth?

l. 177: Figure 2(f-h)?

l. 226: following microstructure measurements (?)

l. 240: the authors should explain here how they define the boundary region (offshore of some given isobaths?)

l. 248-251. I think this sentence needs to be rephrased. Red and blue (empty) bars seem to be always opposite for light density classes, so I am not sure to understand why the authors say that "waters upwell both in the interior and in the proximity of the boundary".

l. 280-283: The "similarity" mentioned here seems to neglect the factor 3 between the inverse estimate and the other ones.

Figure 4: I am probably missing something here. I would have thought that any differences between the four water mass transformation estimates (equation 3, figure 4a), as well as between the four length scale estimates (equation 4, figure 4b), would be due to their different diffusivity or dissipation coefficients. In other words, I would have thought that the buoyancy b, the area of density surfaces A, and the transit time dt, would be identical for each estimate (I cannot find any information on how and from which dataset b and A were computed). Yet, the bulk estimate and the tidally-driven estimate show similar length scales but very different transformation rates for the layer 27.6-27.9 (for instance). Why?

Another point: What is the difference between the green bars on Figure 4a and the filled blue bars (total) in Figure 3b? I thought they were the same (water mass transformation rates for the tidally-driven estimate) but they are not, see for instance the density layer 27.9-28.05.

l. 315: which decade? please give year range.

l. 319: How was the 48{degree sign}N-to-32{degree sign}S distance computed?

l. 329-330: one can wonder whether those near-bottom "very large values" are realistic or not.

l. 385-386: this increase toward the northward flow is not obvious to me. Could the authors precise a bit more what they see?

l.417: "modestly but significantly" sounds a bit contradictory to me (unless "significant" has here a statistical meaning but I doubt so since uncertainties are omitted in this analysis).

l. 431-433: sparse sampling of which quantity? tracers?

Reviewer #2

[Comments begin on next page.]

Review of "Significance of diapycnal mixing within the Atlantic Meridional Overturning Circulation" by Cimoli et al.

Summary

Diapycnal mixing is critical to maintain the current state of the Atlantic Meridional Overturning Circulation (AMOC). Its quantitative contribution to the AMOC and its influence on deep ocean tracer transport, however, are less clear. Using a suite of observation-based estimates of dissipation rate or diffusivity coefficient, Cimoli et al. quantify the diapycnal transformation of the deep waters induced by mixing. They further analyze the impact of diapycnal mixing on tracer pathways in a zonally-averaged model. Results from the study underline the importance of diapycnal mixing in the AMOC and tracer (e.g. oxygen, heat, carbon) distribution, both of these processes are important for the climate.

The manuscript is well-written and the study's focus is of great interest to many people in oceanography. The relevant estimates from a variety of observation-based datasets are quite intriguing. I would recommend publication after addressing comments listed below.

Major comments

[1]. One of the key results of the manuscript is the quantification of diapycnal transformation using different dissipation rate datasets. Because the reported net transformation is quite small (0.8 – 4 Sv) compared to the total diapycnal upwelling and downwelling (~20 Sv), one would wonder if the estimated net transformation (i.e. the difference between upwelling and downwelling) is statistically significant. Thus, while I understand the challenges, I still think it is necessary to make an effort on estimating the transformation uncertainties in order to make the conclusion, i.e. diapycnal mixing is significant within AMOC, more convincing.

[2]. In section 3.1, the authors make a nice argument on the spatial (horizontal) variation of diapycnal mixing and propose that this mixing has a critical impact on tracer distribution. However, in section 3.4, the diapycnal tracer transfer is analyzed using a 2-D (i.e. zonally-averaged) model. I wonder if the 2-D model, even run with bottom-enhanced diffusivity, is representative of the actual tracer distribution. In other words, does the spatial variation of diapycnal velocity shown in Figure 3 affect your results in Figure 6?

Minor comments

[1]. Line 26: Please add "… the Atlantic Meridional Overturning Circulation *(AMOC)*".

[2]. Line 28: It would be helpful to specify the isopycnal associated with the 4 Sv transformation. In addition, is 4 Sv a large portion of the total NADW upwelling? I suggest to report the contribution in percentage of this 4 Sv to the total.

[3]. Lines 31-33: It is quite difficult to understand this long sentence without reading the manuscript. Please re-word.

[4]. Line 203: There is an extra "*is*".

[5]. Line 206: I am not sure if I understand the phrase "*within* the AMOC". What about "*associated with* the AMOC"?

[6]. Figure 3c: The comparison between boundary and interior diapycnal transformation is very interesting. If I understand correctly, the scattered positive diapycnal transformation along MAR in Figure 3b adds up to ~20 Sv, whereas the negative transformation over the much greater interior basin adds up to ~14 Sv. This means the along-topography upward diapycnal velocity must be much greater than the downward velocity in the interior. This difference is not discernible in 3b.

Also, the net transformation is a magnitude smaller than the total diapycnal upwelling and downwelling. It makes me wonder how significant this net transformation is, i.e. whether it is significantly different from 0. Please see my major comment.

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[Version not sent out to review.]